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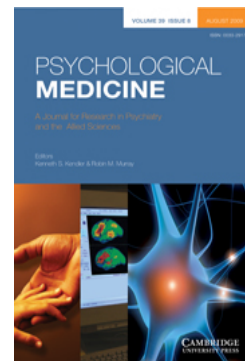
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## Letter to the Editor: Chronic fatigue in Gulf War veterans: should it be treated as chronic fatigue syndrome?

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Psychological Medicine / Volume 39 / Issue 08 / August 2009, pp 1401 - 1402  
DOI: 10.1017/S0033291709005698, Published online: 23 April 2009

Link to this article: [http://journals.cambridge.org/abstract\\_S0033291709005698](http://journals.cambridge.org/abstract_S0033291709005698)

### How to cite this article:

HANS KNOOP, JOS W. M. VAN DER MEER and GIJS BLEIJENBERG (2009). Letter to the Editor: Chronic fatigue in Gulf War veterans: should it be treated as chronic fatigue syndrome?. Psychological Medicine, 39, pp 1401-1402 doi:10.1017/S0033291709005698

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## Correspondence

*Psychological Medicine*, 39 (2009).  
doi:10.1017/S0033291709005698  
First published online 23 April 2009

### Letter to the Editor

#### **Chronic fatigue in Gulf War veterans: should it be treated as chronic fatigue syndrome?**

In their well designed two-phase cohort study, Ismail *et al.* (2008) found that a substantial number of UK veterans of the Gulf War of 1990–1991 who report (medically unexplained) physical disabilities meet the criteria for chronic fatigue syndrome (CFS). There appears to be a considerable overlap between CFS and the so-called Gulf War syndrome as this study and others reviewed by Ismail and colleagues show. This does not seem to be limited to veterans of the Gulf War. We found that 17% of Dutch (ex)-serviceman who were deployed in the 1992–1993 UNTAC peace operation in Cambodia were also severely fatigued. They also reported a high frequency of other CFS symptoms, like unrefreshing sleep, joint pain, and difficulty concentrating (De Vries *et al.* 2000).

CFS can effectively be treated by cognitive behaviour therapy (CBT) that is aimed at the behaviour and cognitions that perpetuate the fatigue and disabilities (Price *et al.* 2008). Ismail and colleagues suggest that the same model could also be used for those Gulf War veterans who have CFS-like symptoms. Indeed, the study of Donta *et al.* (2003) showed that CBT aimed at a gradual increase of activity and improvement of coping and problem-solving skills was effective in treating symptomatic Gulf War veterans. However, this study found only moderate positive effects on physical functioning compared to the effects of CBT for CFS patients. We suggest that this difference may be explained by the fact that for the treatment of CFS, a specific model of maintaining factors with specific interventions is used. In the trial of Donta *et al.* (2003), a rather non-specific form of CBT was offered to symptomatic Gulf War veterans.

That not all (medically unexplained) chronic fatigue is maintained by the same behaviour and cognitions is not new. Gielissen *et al.* (2006) developed a model for chronic fatigue in cancer survivors that was different from the model of perpetuating factors used for CFS patients. Perpetuating factors that were specific for cancer survivors – like fear of disease recurrence or

coping with the experience of cancer – were included in this model. CBT based on this specific model of post-cancer fatigue proved to be highly effective in treating chronic fatigue in cancer survivors.

It is likely that the CFS-like symptoms of veterans with Gulf War syndrome could be more effectively treated if CBT also addresses the perpetuating factors that are specific for the ‘post-deployment syndrome’ (De Vries, 2002). Examples of these specific factors are the subjective experience of the mission, the attribution of somatic symptoms (like malaria medication and vaccinations in the case of Dutch Cambodia Veterans), changes in career perspective, and the perceived lack of attention, recognition, and respect. CBT aimed at these factors combined with interventions directed at the more non-specific perpetuating behaviours and cognitions (e.g. low physical activity, disturbed sleep–wake cycle, low self-efficacy, focusing on bodily symptoms) is probably more effective in reducing ‘CFS-like’ symptoms in veterans with a post-deployment syndrome.

### Declaration of Interest

None.

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### The authors reply

We thank Knoop and colleagues for their comments and we are pleased that they also found high rates of chronic fatigue and related conditions in the Dutch military personnel who were deployed in a different conflict. Knoop and colleagues go on to state that it is insufficient to use conventional CBT for CFS intervention in military personnel; as our study was not an intervention study, this statement does not directly apply to our findings. We suggested a generic CBT model as a starting point but we agree it is important in the development and evaluation of an intervention, which is either completely novel or being applied to a new setting, that it takes account of illness-specific attributions, beliefs and behaviours. We would further add that these specific components should be explicit in any description of the intervention and analysable in a process evaluation (Clark, 2004). Whether a new intervention has to be designed for the psychological sequelae following every new deployment will present a challenge for researchers and for service providers trying to translate generic models to the specific setting.

### Declaration of Interest

None.

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*Psychological Medicine*, **39** (2009).

10.1017/S0033291709006047

First published online 29 May 2009

### Letter to the Editor

#### Response to 'The social determinants of psychosis in migrant and ethnic minority populations: a public health tragedy'

Morgan & Hutchinson's (2009) timely review on the social determinants of psychosis in ethnic minority groups is the latest in a series of papers challenging the assertion that such high rates are related to 'institutional racism' in psychiatry (Singh & Burns, 2006; Murray & Fearon, 2007; Singh, 2007). A high rate of psychosis in immigrants is not a new finding, having been first reported in the 1930s by Odegaard and since then repeatedly replicated. Even some of the architects of the 'institutional racism' claim have started accepting that there is indeed a very high incidence of psychosis in ethnic minorities, calling it 'an epidemic' (McKenzie, 2007). The social aetiology of psychosis in migrants is also not novel. More than 20 years ago, a Canadian Task Force on Mental Health of Immigrants concluded that 'while moving from one country and culture to another inevitably entails stress, it does not necessarily threaten mental health. The mental health of immigrants and refugees becomes a concern primarily when additional risk factors combine with the stress of migration' (Canadian Task Force, 1988). In Britain, however, the ideological drive that places the cause of such higher incidence firmly but erroneously within psychiatric practice has led to years of neglect both of the unmet need of ethnic minorities and the political imperative needed to address the genesis of social disadvantage.

The authors also rightly point out that conflating issues around service use and access with population rates of the disorder has impeded the development of a single policy initiative aimed at reducing high rates of psychosis. However, problems of high rates and poor access are linked in as much as that the attribution of all ethnic differences to psychiatric racism has driven a wedge of mistrust between services and ethnic minority patients. Poor access to care is also related to socio-economic and cultural factors (Singh *et al.* 2007) and any strategy that deals with reducing high rates can not and should not stay divorced from actions to improve pathways into care.

Morgan & Hutchinson place the problem as occurring 'in society'. This is of course broadly correct. Deprivation in the Black Caribbean community in the UK is longstanding, stretching back to the immediate post-war period and even before. A House of